The Behavior Analyst 2010, 33, 37–45 No. 1 (Spring)

Behavior Under the Microscope: Increasing the Resolution of Our Experimental Procedures

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Behavior analysis has exploited conceptual tools whose experimental validity has been amply demonstrated, but their relevance to large-scale and fine-grained behavioral phenomena remains uncertain, because the experimental analysis of these domains faces formidable obstacles of measurement and control. In this essay I suggest that, at least at the fine-grained end of the behavioral spectrum, we have not taken sufficient advantage of all available procedures. Specifically, I propose that an examination of eye movements, joint control, and response latency in intraverbal tasks might help us to formulate more complete accounts of complex human behavior.

Key words: eye tracking, interpretation, joint control, private events, relational behavior, semantic priming

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I am struck by the range of topics discussed by contemporary behavior analysts and particularly by the different scales of such analyses. Some behavior analysts, for example, are interested in cultural evolution and the metacontingencies that apply to groups of people, often over extended periods of time (see, e.g., Glenn & Malott, 2004; Hull, Langman, & Glenn, 2001; Skinner, 1981). At the other extreme, some behavior analysts engage in interpretive exercises that require consideration of covert behavior or behavioral processes that occur over tens of milliseconds (e.g., Donahoe & Palmer, 1994/2007; Palmer, 2009; Schlinger, 2008). Most behavior analysts find themselves somewhere in between, perhaps studying organizational behavior, human behavior in applied settings, or animal behavior in tightly restricted settings.

Not all topics of interest are equally amenable to experimental investigation. In general, the larger the scale of interest, the more variables are at play, and the less control one has over subject histories. One cannot easily manipulate cultural practices systematically in a natural setting. At the other end of the scale, very small behavioral events are often equally difficult to measure and observe. If one asks a child, "How many spoons are on the table?" the response "seven" is presumably the terminal response of a systematic sequence of orienting and counting responses that are usually not recorded and, because of their reduced scale, are very difficult to measure. In this case, scrutiny, together with instrumental amplification, might reveal some of these behavioral events, because orienting and counting, even if covert, are relatively discrete events that can be assumed to occur in an orderly sequence and that serve a clear purpose in the task from moment to moment in time. But suppose one had asked "How many spoons were on the table this morning?" The response "seven" would presumably still be the terminal response of some sequence of behavior, but it is not easy to say what the elements of that sequence are. Generally speaking, all research with human subjects is bedeviled by uncertainty about their histories. Those

I thank Joe Layng, Hank Schlinger, Greg Stikeleather, Janet Twyman, and Elisa Hegg for indirect contributions to my formulation of the points in this paper, and I thank Tim Fuller for inviting me to participate in the forum that forced me to put them together.

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who wish to study organizations, cultures, and other phenomena at the macroscopic end of the scale face the additional problem of controlling relevant independent variables. Research on molecular events in human behavior faces, in addition to problems of history and control, the problem of the difficulty of observing the phenomena of interest.

Phenomena best suited for experimental analysis lie not at either extreme on the scale, nor even with human behavior at all, but in the animal laboratory with relatively discrete, observable motor responses. The strategy adopted by behavior analysis is that of all other sciences, namely, to study nature closely whenever possible, to extract general principles from that study, and to extrapolate to domains where experimental control is less congenial. Consequently, most of our conceptual tools, our analytical tools, and our research methodology can be traced directly or indirectly to the animal laboratory. It is a sound approach and has served the field well, because the principles formulated in the animal laboratory appear to be relevant virtually everywhere one looks in the domain of human behavior.

The success of standard operant methodology has led to a conservative tendency to ignore the relevance of work in other paradigms and a failure to fully exploit some experimental procedures that have not been pioneered in the operant laboratory. In this paper I will briefly discuss three dependent variables that are of particular interest to me, at the relatively molecular end of the spectrum of behavioral phenomena, and I will suggest how an expanded repertoire of experimental and analytical tools might permit us to make progress in our interpretations of such phenomena. I offer these as cases in point to suggest that the future of behavior analysis may lie in the expansion of our experimental procedures. I will consider, in turn, eye movements, fluctuations in response strength as a controlling variable, and response latency in intraverbal behavior.

To whet the reader's appetite for a consideration of new experimental procedures, consider how formidable is the interpretive task of explaining even the most commonplace examples of human behavior: Yesterday I answered the telephone to hear a recording of Bill Clinton urging me to vote for the Democratic candidate in the U.S. Senate race occasioned by the death of Ted Kennedy. An observer would have reported little behavior in me during the call and no apparent reinforcers, but today I can report the call in some detail. Our behavior is continually being conditioned relative to some context, the conditioning occurs over the course of hundreds of milliseconds, and often there is no conspicuous evidence of the relevant behavior or of relevant reinforcement. It is just such considerations that breathe life into normative cognitive science. If we are to displace conventional interpretations of such phenomena, we will need to offer cogent competing accounts; to do so, we must make use of all available tools and consider all relevant dependent variables. It is important to note that even if a behavioral interpretation cannot be confirmed experimentally at present, it still serves a useful purpose in staking out a claim to the domain of interest and in integrating otherwise fragmentary experimental data into a coherent account.

Eye Movements

As a dependent variable, eye movement has been exploited only rarely by behavior analysts (e.g., Dube, Balsamo, Fowler, Dickson, & Lombard, 2006; Rosenberger, Fischman, & Goldiamond, 1969; Schroeder & Holland, 1968a, 1968b, 1969), perhaps because eye movements themselves are of less interest to researchers than the discriminative responses that follow. Moreover, eye fixation is an imperfect indicator of these other responses. However, the technology is widely used by web designers, human factors engineers, and cognitive psychologists, in the latter case often in the service of models of language processing in reading (see Rayner, 1998, for a review). Because the dependent variable is behavior, it is worth asking whether eye movements can be recruited to supplement other behavioral observations in the service of interpretations of cognition that rest, not on hypothetical constructs, but on principles and concepts rooted in an experimental analysis.

To take one case in point among many, consider the question of what controls tacts of relations among visual stimuli. Specifically, are such tacts controlled directly by stimulus relations or by our responses to such relations? The answer to this question will have far-reaching implications for our understanding of much complex behavior. When I remark that "the black square is left of the red triangle," it is tempting to suppose that the crucial controlling variable is the relation between the stimuli. Indeed that interpretation is so self-evident that it is likely to be assumed without question, and as a practical matter that interpretation is likely to be most expedient for adult humans. But adult human behavior is often so effortless and swift that its complexity may be obscured. I like to apply the following test to determine if I really understand something: Can I imagine, at least in principle, how I might train a neural network to perform the task, given the condition that the network is informed only by principles of behavior and physiology (Donahoe & Palmer, 1989)? Such models invariably make many simplifying assumptions, particularly about the response of the system to stimuli, so an answer of "yes" is no occasion for self-congratulation, but an answer of "no" is therefore all the more troublesome. In this case one faces the question: What are the stimulus properties of *left of*? In a world in which X is always left of Y, the relation might be learned as a monad, but we need to explain generalization to novel examples of X and Y, so we cannot appeal to a retinal pattern. Indeed, the performance would seem to require *left of* receptors, and as far as we know, there are no such receptors. This, of course, is just a case in point of relations in general, leading me to the conclusion that I do not understand relational responding.

Fortunately, William Hutchison succeeded in precisely this demonstration (Hutchison, personal communication, December 12, 2009). That is, he showed that a neural network that models behavioral principles can learn a generalized tact of the form *X is left of Y*. One advantage of such demonstrations is that when the system behaves in unexpected ways, all of the causal variables can be teased apart moment to moment in time. In this case, Hutchison discovered that the variable that primarily controlled the response *left of* was the element in the simulation that represented stimulus properties of the saccade. That is, an invariant property of the demonstration, from one example to the next, was a shift in foveal vision from one stimulus to the other. One such shift was always correlated with reinforcement of *left of* and therefore acquired stimulus control of that response. This solves the problem neatly. We may not have *left of* visual receptors, but we do indeed have proprioceptive receptors.

That Hutchison's network stumbled on one solution to the problem does not mean there are not others that do not depend on eye movements, because we are very far from being able to itemize all of the behavioral events that are embraced by the loose concept of "interpreting a visual scene." Nevertheless, there is no point in ignoring a measurable dependent variable. The relevance of saccades in a simulation of the behavior suggests that we should monitor eye gaze moment to moment in time with eye-tracking technology in a study of the emergence of such relational responding in typical children, or perhaps children with disabilities.

Of course, any relevance of eye movements in the acquisition of such a task would not explain our responses to other kinds of relations, but it would encourage a search for behavioral events that might serve as invariant elements of relational responding. One example would be covert verbal behavior. Most people would have little trouble indefinitely continuing the series, 0, 7, 14, 21, 28, 35 …. The relation is, of course, "plus seven," but that relation is not "in the stimulus" but in our responses to the stimuli. The invariant element in the relation is a verbal response, often covert, but not necessarily so. Thus, our interest in eye movements lies not in their effect on the external world, or even in their role in one instance of relational responding, but as one element in a comprehensive behavioral interpretation of human behavior.

There are, of course, many other questions that one might investigate with eye-tracking technology: Are eye movements in reading correlated with the arrangement of verbal operants? What is the role, if any, of eye movements in various types of problem solving? When recalling a visual scene, are eye movements correlated with the position of things in that scene? If we interfere with normal eye movements by instructing subjects to follow a dot on a screen, is recall impaired? Although there is a substantial body of research outside behavior analysis on questions of this sort, it is seldom designed in a way to foster a behavioral interpretation.

Fluctuations in Response Strength as a Controlling Variable

In *Verbal Behavior*, Skinner (1957) coined the term *descriptive autoclitic* for a class of verbal operants that specify the nature of the stimulus control of other verbal operants. Consider the following hypothetical utterances:

I think it's a marsupial.
I'm certain that it is a marsupial.
I felt like shouting, "marsupial!"
I was on the point of observing that it's a marsupial.
The word *marsupial* was on the tip of my tongue.

These examples suggest that the strength of a response, across a wide range, is itself discriminable. In the case of an emitted response this is uncontroversial, because there are presumably relevant proprioceptive cues. But when the response is never actually emitted, as in the tip-of-the-tongue phenomenon, or when we are "bursting with news" but are too polite to interrupt the speaker, we are presumably responding to something other than the putative response itself (unless it happens to have been emitted covertly). All vocal behavior requires the coordinated action of the diaphragm, larynx, throat, jaws, tongue, lips, and so on. In such cases it is possible that reports of response strength are controlled by actual changes in some of these response dimensions rather than changes in the probability of unemitted behavior. To put it loosely, we start to take the floor before we begin to speak. Our self-reports of what we were "about to do" are presumably controlled by the constellation of events that actually happened, not those that did not happen. In any case, it seems uncontroversial to assert that we can usually tact the strength of a response either directly or inferentially.

Lowenkron has shown that *joint control* can itself be a controlling variable for verbal responses (e.g., Lowenkron, 1998). Joint control is the simultaneous control of a single response topography by two variables. For example, suppose we are searching through a list of phone numbers to find the name corresponding to a given number. We are likely to repeat the target number to ourselves periodically as we look through the list (self-echoic behavior). As we proceed down the list, we read the numbers (textual behavior). When the topography of the textual behavior corresponds to the topography of the echoic behavior, we have "matched." According to Lowenkron, the onset of joint control is a discriminable event that permits one to determine that a match has been found. That is, it is not the phone number itself that permits us to announce a match but that joint control has been detected. Lowenkron has shown that matching behavior can be trained in children with disabilities by arranging conditions under which joint control will occur (Lowenkron, 1984, 1988, 1989, 1991).

That joint control is sometimes discriminable seems beyond question. No doubt we have all had experiences like the one Skinner reported:

> In the grade school that I attended as a child, a single teacher taught two grades in the same room. While one class recited, the other worked on its assignments. One day in third grade, when the teacher was talking with the other class, I raised my hand, waved it wildly to attract her attention, and said, "I was *reading* the word 'middle' just when you *said* it." (Skinner, 1978, p. 171)

I believe that the critical feature of such phenomena is the saltation in response strength corresponding to the onset of joint control. Moreover, I believe that the discrimination of this feature underlies any behavior that depends on judgments of identity. I take this to be Lowenkron's position as well, and I want to acknowledge his priority. However, because I discuss a somewhat different range of phenomena and in slightly different terms, I don't want the reader to blame him for any excesses or fallacies in my account. The reader may find this to be an unnecessary, if not preposterous, claim, because judgments of identity might seem to be matters of simple stimulus control and simply a kind of generalization. Indeed, so it seems to some of my colleagues to whom I have broached the subject: A judgment of identity is simply a discriminated response to identity in stimuli. But I think this view is wrong. Like other relations, identity is not "in the stimuli." That is, we are not responding to a quality of *identity*; rather, we judge identity according to our responses to the stimuli.

It is easy to show that people's judgment of identity is not controlled by a "property of identity," because they will sometimes judge two stimuli as identical when they are not, that is, when the putative property is missing. Moreover, the very concept of identity is suspect. No two stimuli are identical in time and space. Nor can we simply appeal to a history of reinforced identity matching, because we can generalize to new exemplars of unlimited variety. What are the invariant properties of an event that control the response *identical*? I am arguing that a judgment of identity is controlled, not by a stimulus property of *identity*, but by a common behavioral effect of the two stimuli, that is, that identity is marked by joint control.

However, this argument seems merely to push the problem of stimulus control away from the relatively objective external world to the less easily measured world of "behavioral effects." Moreover, the problem seems to remain as intractable as before: How do we know that two behavioral effects are identical? If identity is not a stimulus property, surely it is not a response property either. But I propose that when we judge two stimuli to be identical, we are not responding to a supposed property of identity but to saltations, or perhaps decrements, in response strength. When we glance from one stimulus to another, our responses to the stimuli either supplement one another or compete with one another. Assuming that summation and competition are discriminable, they could serve as controlling variables for judgments of identity, similarity, or difference. I believe that this proposal is adequate to the problem at hand and that it avoids conundrums that arise when we invent qualities of objects to correspond to the behavioral effects of those objects.

Notice that I am not arguing that stimulus generalization requires such an account. When we learn to name daffodils in the presence of a particular specimen, we may generalize to other daffodils and perhaps to jonquils and some other flowers as well. Daffodils have stimulus properties that might come to control the tact *daffodil* when those stimulus properties vary somewhat, but *identity* does not have stimulus properties that vary along some gradient from one example to another. When we tact two tastes as identical and two pictures of the Mona Lisa as identical, we are not simply generalizing from one case to the other because of a stimulus gradient.

Only rarely are we called on to judge the identity of objects explicitly, but such judgments are entailed by many other phenomena of importance. I have argued elsewhere (Palmer, 1996, 1998) that the "hidden engine" of language acquisition is reinforcement by achieving parity with practices of the verbal community, and any example of generalized imitation seems to rest on detection that one's behavior has matched that of a model. As Skinner (1957) observed, with regard to a child learning an echoic response, "Trying to make the right sound, like trying to find one's hat, consists of emitting as many different responses as possible until the right one appears" (p. 60). Recognizing that the right one has appeared is a kind of identity matching. Such phenomena are of central importance in human behavior, but I don't believe we understand them well. Consequently, I believe that an important future direction of behavior analysis is bringing them into good conceptual order; building on Lowenkron's work on joint control is a good place to start, because it offers a parsimonious explanation of identity matching without appealing to a stimulus property of *identity*.

Response Latency

Response latency and response rate are roughly reciprocal measures of response strength. The latter is clearly the measure of choice in free-operant procedures, because there is no starting point from which to measure latency, but in discrete-trial procedures latency is clearly the more useful measure, because response rate is limited by the rate of stimulus presentation. However, in such procedures, latency is often not recorded with precision, if it is recorded at all, because it is commonly subordinate to other measures that may have more practical importance, such as whether the response was "correct" or whether it happened at all.

There is one context in which response latency seems to be uniquely suitable as a dependent variable, but behavior analysts have been slow to exploit it. I refer to subthreshold additive effects of controlling variables on a single response. As noted in the preceding section, outside the laboratory, it is commonly the case that behavior is determined by multiple variables, perhaps coming together for the first time. Whereas joint control refers to a discriminable increase in the strength of an emitted response, I refer here to fluctuations in strength of a response that has not yet been emitted. At any moment we are bombarded by stimuli, most of which can control discriminative behavior under some conditions. We can assume that the mere presence of such stimuli increases the strength of corresponding responses, even if they are not emitted. The behavior that is actually emitted at a given moment may be the result of the integrated action of many concurrent variables, both evocative and suppressive. Skinner (1957) explicitly adopted this position in *Verbal Behavior* and discussed it at length in chapters on multiple causation and supplementary stimulation. Consider the following excerpt:

> If an echoic or textual stimulus acts when a response is strong for thematic reasons, the probability of emission is increased. The supplementary stimulus may simply cause the speaker to utter aloud a response which has already occurred subvocally. More often, the overt–covert distinction is not at issue. Thus, a forgotten name which is "on the tip of one's tongue" is instantly recalled (not merely read) when the name is seen in glancing at printed matter. In a noisy conversation we may overhear a verbal response which is currently strong in our own behavior, and the response may then "occur to us" although it would otherwise have remained latent. We say that we have been "reminded" of something. (pp. 243–244)

One might reasonably suppose that fluctuations in the strength of latent behavior lie entirely beyond our ability to investigate them experimentally, but the semantic priming literature suggests otherwise. Semantic priming is a wholly behavioral procedure, but it has been exploited hitherto almost entirely by cognitive psychologists. (For empirical work with the procedure within behavior analysis, see Barnes-Holmes et al., 2005; Haimson, Wilkinson, Rosenquist, Ouimet, & McIlvane, 2009; Hayes & Bisset, 1998; and Palmer & Katz, 2005. For a conceptual discussion of the procedure from a behavior-analytic perspective, see Branch, 1994; Donahoe & Palmer, 1994/2007; and Palmer, 2009.)

As it is commonly implemented, the priming procedure assesses the latency to respond to the second of two briefly presented stimuli. The typical finding of such experiments is that if the two words are intraverbally related, as in *table* and *chair* and *bread* and *butter*, response latencies to the second stimulus are shorter, on average, than if the stimuli are unrelated, as in *bread* and *table*.

These findings suggest the following interpretation: *Bread* is a textual stimulus that presumably evokes the corresponding subvocal textual response, *bread*. In addition, the textual stimuli, or perhaps the stimulus properties of the textual response, or both, potentiate, but do not evoke, the intraverbal response *butter*. That is, one does not actually emit *butter*, either overtly or subvocally, but it increases in strength. When the textual stimulus *butter* appears, it evokes the textual response, *butter*, but the response latency is shorter because of the intraverbal potentiation of the response by *bread*. In principle, it appears that the procedure permits a quantitative assessment of extremely subtle effects of textual stimuli, and presumably other stimuli as well.

No doubt such effects vary according to one's history with the stimuli, and here lies another possible future direction for behavior analysis. What is the intraverbal effect of a single pairing of two arbitrary verbal stimuli as either auditory or textual stimuli? If none, how many pairings are necessary before an effect is observed? Does it matter if the stimuli can be echoed (i.e., are speech sounds in the subject's verbal community)? What is the effect on response potentiation of using meaningful stimuli or stimuli that evoke vivid imagery? Because of the indirect nature of the data, it is uncertain that such questions can be answered with confidence, and it may be that the answers vary so much with subjects' histories that there are no general answers. Nevertheless, the additive effects of stimuli on response strength are a consideration that must be taken into account in almost all human behavior, because multiple control is the rule, not the exception. The more we understand about the effects of verbal stimuli on other responses, the better our interpretations of complex behavior will be.

A second finding of interest in the semantic priming literature is that categorical terms, like *fruit*, potentiate many exemplars of the category, such as *apple*, *banana*, and *peach*. Because experimental subjects do not know in advance what stimuli are going to be presented on any given trial, it follows that the potentiation of all of the exemplars of a category must be simultaneous. Although not surprising, this finding is important in showing that a single verbal stimulus has multiple effects on the repertoire. From this it follows that normal speech, typically delivered at several words per second, has incalculably complex effects on the listener's repertoire, with countless mutually incompatible intraverbal responses rising and falling in probability, stirred up anew as each succeeding stimulus is presented. But order emerges from this bedlam, because the behavior of the experienced listener is ordinarily coherent, unitary, and relevant. Our interpretive schemes must accommodate both the complexity of the molecular behavioral events and the relative simplicity of the resulting integration of these events. I believe that the priming procedure is a tool that can help us make sense of these seemingly incompatible observations (see Palmer, 2009, for further discussion of these points).

Conclusion

Most of the conceptual tools of the modern behavior analyst were forged in the animal laboratories of the 1930s, and their continued relevance shows how well they were made. Using just those tools, Skinner offered accounts of almost every aspect of human behavior, and they remain fresh, inspiring, and satisfying. However, his accounts were not the end of inquiry but rather the beginning, because many thorny problems in the interpretation of behavior remain. In this essay I have suggested that one way we might advance is to expand the scope of our dependent variables, in some cases taking advantage of contemporary experimental techniques that are commonly used by others.

Eye movements have no direct effect on the environment, but they clearly serve other behavior in important ways. They might provide a way of indexing various discriminative responses presumed to be necessary components of complex behavior. In addition, they might help resolve questions about the nature of controlling variables in relational responding, a topic that I am persuaded is more difficult than commonly assumed. Joint control is not a new topic, but its role in complex behavior is only beginning to be appreciated. I believe that it offers a parsimonious explanation of a fundamental element of most complex performance, namely, discriminating identity. As with other relational responding, I think the problem of interpretation is challenging, and I think Lowenkron's work should be followed up. The semantic priming procedure appears to me to provide at least a narrow aperture through which we can observe the buzzing, blooming confusion of subthreshold fluctuations in response strength. Because behavior analysts have a set of coherent conceptual tools that have emerged from a century of experimental analysis, I believe we will be able to use such data to maximal interpretive effect.

If our field is to displace standard accounts of commonplace human behavior, we must not shrink from the formidable task of interpreting it in behavioral terms. That we cannot do so with the sureness with which we explain much other behavior is no cause for embarrassment, because the difficulties of measurement, control, and accessibility are limitations for everyone, not just behavior analysts. Nevertheless, our interpretive task will be easier if we are able to take advantage of all of the available data. I have suggested three ways in which we might do better.

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